may signalise those of the stomach and those of the kidney—the structure of the last-named organ being illustrated with particular minuteness. One is glad to think that one's examinations are over on finding that there are now no less than sixteen several named parts to be remembered in describing the course of a uriniferous tubule!

We have hitherto been writing as if the book before us were an Atlas of Plates and their description, and nothing more. This is emphatically not the case however, for the plates are accompanied by a text written by Dr. Klein, which forms a complete and independent compendium of the present state of histology, giving in plain terms and as briefly as is consistent with clearness, an account of the minute structure of each tissue and organ. In this account credit is given wherever possible to those to whom the discovery of new facts is due, but it is, we think, to be regretted that the references to the works in which the facts were published has not been added; such a notice of the literature of each subject would have been of much value.

At the end of the book a description of the appearances which are presented by nuclei in process of division will be found, embodying the results of the recent researches of Strasburger, Flemming, Mayzel, Klein, and others, results which have not unnaturally created a feeling of wonderment that in objects which have long engaged the special attention of histologists, changes of so marked a character should occur, and until now have wholly escaped observation.

We see that endothelium is still described as a tissue distinct from and indeed in contradistinction to epithelium, but it seems to be upon its last legs, for it now has to depend for existence upon a negative definition, and no longer presumes to base its claims to the place upon its developmental history.

There is a general tendency throughout the text to teach the subject somewhat dogmatically, and this, with the absence of detailed reference to literature, detracts from its value as a work of reference, while perhaps increasing its value as a text-book for students. Taking the work however as a whole it is not too much to say that it is in every way worthy of the high reputation of its principal author, and that its appearance, supplying as it does a want that has been long felt, will be welcomed by histologists both at home and abroad.

OUR BOOK SHELF

Urania: an International Journal of Astronomy. Edited by Ralph Copeland, Ph.D., and J. L. E. Dreyer, M.A.

THE first number of what is intended to be a high-class astronomical periodical, with the above title, has just appeared, and forms twenty-four pages demy quarto. It is proposed to issue it in numbers of from sixteen to twenty-four pages, whenever sufficient material offers, with shorter numbers when subjects of immediate interest require it. Papers will be accepted and printed in French, German, and Italian. This first number is well supported. It contains an article on the solution of Lambert's equation by Prof. Klinkerfues, and auxiliary tables for the calculation of occultations of stars by the moon, by Dr. C. Borgen. The Earl of Crawford and Balcarres contributes observations of comets 1880 *b*, *c*, and *d* made at Dunecht; the Earl of Rosse has a paper on determinations of lunar radiant heat during an eclipse; and Dr. R. S. Ball communicates an investigation of the parallax of the star Groombridge 1618, which is No. 89 in Argelander's list of stars with large proper motions, the observations having been made at Dunsink in 1878-79: a parallax to the amount of a third of a second is indicated by the measures both of distance and position, so that, as Dr. Ball remarks, there is reason to consider Groombridge 1618 entitled to a place amongst the sun's nearest neighbours. Dr. Copeland has a note upon a nebula detected at Dunecht on the method of sweeping suggested by Prof. Pickering of Harvard College, U.S., which is termed "a new planetary nebula." The nebula however is not new; it was discovered several years since by Mr. S. C. Burnham with a refractor of 6-inches aperture, and was notified at the end of his third catalogue of new double-stars: it is referred to also in the notes to his observations of double-stars in 1877-78, in the *Memoirs* of the Royal Astronomical Society, vol. xliv.: he found it to be double, the distance between the centres of the two parts and a star of 9m. (which appears to be *Durch.* + 47°, No. 3289), being 27″·3 at the epoch 1878·47. The double nucleus has also been remarked at Dunecht, and the measures of position and distance made there have a particular interest when compared with Mr. Burnham's in 1878; thus we have—

Burnham, 1878·476 ... Position, 88°·5 ... Distance, 2″·57
Dunecht, 1880·913 ... „ 71·9 ... „ 8·00

Such differences surely indicate rapid motion. Dr. Copeland does not allude to the star of about ninth magnitude distant less than half a minute of arc in 1878. This journal may be obtained by applying to Mr. J. L. E. Dreyer, Observatory, Dunsink, Co. Dublin.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

Infusible Ice

PROF. CARNELLEY'S directions in NATURE, vol. xxiii. p. 341, just received, for producing the hitherto fabulous commodity, "hot ice," have succeeded so much beyond our expectations for a first experiment in our College laboratory to-day, that the ease and simple means with which the experiment was performed, and the unaccountable and unaccustomed appearances which it presented, recommend it very strongly, as Prof. Carnelley remarks in his paper, and as I hope that the following description may also serve to prove, to other observers' trials and repetitions.

A 30 oz. flask of stout glass (made nearly as strong as the Carré decanters, for vacuum experiments) was tightly fitted, by forcing a two inch plug of large solid india-rubber tube placed round a tube into its mouth and firmly fixing it there with wire, with a delivery-tube of three-eighths of an inch large glass barometer-tube about two feet long. This tube was bent into an S-shape, and at the extreme end of the small U-part, which turned up, it was drawn out to a nearly capillary neck and bent over to communicate by india-rubber tubing through another similar flask surrounded by cold water to act as a condenser, with a Swan's aspirator giving a vacuum of twenty-eight inches by the action of the town water-supply. About 15 oz. of distilled water previously introduced was now boiled in the flask *in vacuo*, and distilled over, at a very gentle heat, into the second flask. After two or three hours' boiling the quantity of water in the flask was reduced to about 3 oz., and the capillary end of the tube was then sealed with a blowpipe. The flask thus exhausted makes an excellent water hammer and cryophorus, ringing with sharp raps when the water is shaken to and fro in it or in its tube. The least warmth of the flask, or cold applied to the U-part of the tube, suffices to collect there, by distillation, a quantity of beautifully clear water completely freed from air.

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To obtain the rapid evaporation and superheated state of ice, the U part of the tube was placed in a freezing-mixture of ice and salt till the condensed water coated the tube internally with a hollow sleeve of ice extending in the long and short parts of the U to a total length of about eight inches, and the flask itself was thereupon placed in a sawdust-bedded tin pail containing a large freezing-mixture of 5 lbs. of salt and 10 lbs. of pounded ice. Hydrochloric acid was at the same time added to the frigorific mixture round the U-tube, lowering the temperature of that part of the cryophorus to – 29° C. (– 20° F.), which had the effect of cracking the ice-sleeve (apparently by contraction) in all directions, giving rise at first, from its appearance, to the apprehension that the glass tube was completely splintered! Now came the critical operation. Would the hard frozen ice-coat bear the application of the heat ordeal without melting?

The freezing mixture round the U-tube was replaced by cold water in a water-bath, which was heated rapidly with a Bunsen-flame. About two inches of the ice-sleeve in the long part of the tube stood above the surface of the water; the rest soon melted, while the temperature of the water in the bath rose to 70° C., and a thick rime of white frost from the condensed vapours of this water formed round the tube containing the projecting part of the ice-tongue, which must have been intensely cold, since the rime almost touched the surface of the warm water. Below the surface of the water the tube appeared always to contain some snowy-looking solid, along with what resembled water proceeding from its liquefaction. When the temperature of the bath reached 70° C. the projecting ice-tongue was still unmelted; it had replenished itself by condensing and freezing the vapours rising up to it from below, and formed a snowy plug in the tube an inch or more long.

The water-bath was then removed in order to heat this snow-plug with a Bunsen-flame, and to our extreme surprise a similar snow-lining was found still to remain coating, quite dry, the whole long part and much of the short part of the U-bend. The flame was applied, and the whole tube was heated violently without for some time appearing to have the least effect upon the white crust within, notwithstanding the tube was too hot to be touched! A small flake weighing at most a few tenths of a grain, at the bottom of the U, withstood the strongest heat there for several minutes, wasting insensibly away, and unchanged in shape, until it vanished with perhaps a moment's collection to a drop as it disappeared. As it slowly grew thinner the white coating in the tube seemed in general to be no more affected by the heat than white feathers would have been; but in particular parts there often occurred partial liquefaction or pasty fusion allowing pieces of the film to turn over by their weight and roll or slip down the glass while still adhering to it, quite in the manner of drops. That the liquid itself, completely freed from air, refuses to boil, and may be described in that respect as practically oily, was evident enough in the preparation of the flask, when except by sudden bumps the distillation of vapour, however much it was urged by heat, and seen to be passing copiously through the bent worm-tube, proceeded almost entirely from the surface of the water in the flask.

In these drops at least, adhering to the heated glass, it seems difficult to believe that the solid ice which makes them angular, jagged in form, and pasty, can be anything but superheated; and it seems also scarcely credible that the latent heat of sublimation of the insignificant weight of a few grains of ice-crust in the tube resisting its strong heat for many minutes can reach the large amount of its gain of heat in that time by conduction and radiation from the surrounding tube, if the ice-film is at no higher temperature than its natural melting-point. These are questions however which the calorimetric methods devised and pursued by Prof. Carnelley are best designed to answer, and to which replies without such positive determinations can only be urged at present as probable conjectures, or as clear preceptions and presentiments, on the other hand, of the action and operation of some hitherto undiscovered thermal laws.

Whether by direct sublimation, or by evaporation accompanied by liquefaction, the slender snowdrift with its enlargement at the top was gradually reduced in thickness, and subdivided into remaining parts along its length, which all adhered to the tube until they wasted away, and the largest top piece never fell by withdrawal of support below, but, like the rest, it clung like a thin scale to the last to its original place on one side of the tube. This power of adhesion of hermetically volatilised ice to hot bodies with which it is in contact forms, as Prof. Carnelley observes, one of its most marked, although not at all one of its most prognosticable, properties, and it certainly prompts, if it does not unequivocally substantiate, the supposition that the ice in such close contact with extremely hot bodies must be superheated, or at a temperature sensibly higher than its natural melting-point.

The perfect success of the experiment and the beautiful appearance of the ineffaceable white snow-lining of the tube afforded us unqualified pleasure and surprise, and the simple preparation and preservation of the cryophoric apparatus needed for its exhibition will, I have no doubt, cause the experiment to be often reproduced and shown wherever freezing-mixtures and the cryophorus, and Boutigny's and Leidenfrost's phenomena, and Prof. Carnelley's theory and researches, are subjects of lecture-demonstrations.

A. S. HERSCHEL

College of Physical Science, Newcastle-on-Tyne, February 15

Dust, Fogs, and Clouds

I NEED not say that the information contained in M. H. J. H. Groneman's letter in NATURE, vol. xxiii. p. 337, was a most unexpected surprise. Nothing whatever seems to have been known in England about the results obtained by Messrs. Coulier and Mascart; and my astonishment was not diminished when I considered that their important investigation had borne no fruit, never having been practically applied by meteorologists or others.

I have just seen the article by M. Coulier in the *Journal de Pharmacie et de Chimie*, and will (with your permission) make a few remarks on his paper and on my own. M. Coulier was led to the discovery of the important part played by dust in the condensation of vapour, by making experiments on the well-known cloudy condensation produced by expanding saturated air. Instead of the ordinary air-pump arrangement M. Coulier's apparatus consisted of a glass flask, in which was placed some water. This flask was connected by a tube to a hollow india-rubber ball. He first compressed the india-rubber ball, thus compressing the air in the flask. The pressure was then removed from the ball, when the air in the flask expanded, and the condensation became visible.

In making repeated experiments with this apparatus M. Coulier noticed that the action of the air was capricious. After the air had remained in the flask some days he found it quite inactive. He also found it inactive after shaking it up with the water in the flask, and that on expelling some of the inactive air and replacing it with the air of the room the cloudiness again appeared. He then found that if he filtered the air before admitting it to the flask it did not give any cloudiness when expanded, and he explains with perfect clearness that the dust in the air formed the nuclei on which the vapour condensed.

Having explained the manner in which M. Coulier approached the subject I shall now give the history of the corresponding part in my work and show the direction from which I approached it. I had been studying the action of "free surfaces" in water and other forms of matter, when changing from one state to another. I knew that water could be cooled below the freezing-point; I was almost certain ice could be heated above the melting-point, and I had shown that water could be heated far above the boiling point; that the material of which the vessel holding the water was composed had no influence on the result—and all that was necessary to prevent the change of state taking place, at the freezing and boiling points, was an absence of "free surfaces" at which the change could begin. Arrived at this point, the presumption was very strong that vapour could be cooled below the "condensing point" without changing to water, if no "free surfaces" were present. I first intended to experiment with steam at the pressure of the atmosphere, but found difficulties in operating at so high a temperature. I then made arrangements to conduct the experiments at a lower temperature, and for convenience experimented on steam mixed with air. I then saw that dust in the air would form "free surfaces" on which the vapour would condense. I therefore attached a cotton-wool filter to the apparatus, and filtered the air before it entered the receiver. When this was done I found that the steam on entering the receiver was perfectly invisible, and gave rise to no cloudy condensation, the air remaining supersaturated. The experiment was immediately afterwards repeated somewhat in the same way as was done by M. Coulier, the air being supersaturated by expanding it by means of an air-pump.

Though the two investigations approached the subject from very different points, they seem afterwards to have flowed in almost parallel directions. Starting from his first experiments,

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